written narrative of Australian discovery interesting. Mr. Eden has told the story attractively, and the reader will not only be greatly interested, but will have a fair idea of what has been done to extend our knowledge of the "fifth continent" from its first discovery down to the trans-continental journeys of Warburton and Forrest—the latter, however, being referred to in a sentence or two.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

Dr. Bastian and Prof. Tyndall on Spontaneous Generation

I BEG you to allow me a few lines to protest, as Prof. Tyndall has done elsewhere, against Dr. Bastian's proceeding, in citing a number of observers *in support* of his views (NATURE, vol. xiii. p. 284), whose researches taken in each case—as a whole—furnish conclusive arguments *against* his views.

It is only by an inadequate statement that the observations of Dr. Pode and myself—which appear in Dr. Bastian's list—can have this signification attached to them. Where we obtained the result which Dr. Bastian obtained, we were able to trace it to a vitiation of the experimental conditions. Our results conclusively and categorically contradicted the particular assertions contained in Dr. Bastian's book, the "Beginnings of Life," into the truth of which we set ourselves to inquire.

Feb. 16 E. RAY LANKESTER

Radiometers and Radiometers

I HAVE recently been trying some experiments with a radiometer, obtained from Mr. Browning, and as some of my results are different from what I was led to anticipate, I should like to know whether there is anything special in my particular instrument, or whether other people have noticed the same things.

In Mr. Crookes' paper on "The Mechanical Action of Light," *Quarterly Journal of Science*, No. xlvii. p. 348, he states that "when only dark heat is allowed to fall on the arms [of the radiometer], as from a vessel of boiling water, *no rotation whatever is produced.*" (The italics are mine.)

Now I find that my radiometer is particularly sensitive to dark heat, the presence of a heated copper wire, or still more that of an iron poker when only slightly warmed, instantly accelerating the number of revolutions.

But more than this: when exposed in a room to diffused daylight, the velocity of rotation is greatly influenced by the temperature of the room, and is by no means an indicator of the amount of light only.

One morning this week during the frost, upon looking at my radiometer, it appeared to be motionless, although standing not far from my study window. When placed nearer to the light it revolved, but so slowly that I thought the instrument must have received an injury. The room at the time was very cold, as the fire had not been lighted. After the fire had been lighted and the temperature of the room raised, the velocity of rotation increased, and upon observing the instrument just before dark, when the room was very warm, the rotation was considerably greater than it had been in the window in the middle of the day, although at the time there was only just enough light in the room to enable me to see the instrument at all. When I brought the radiometer near to the fire, which consisted only of dull hardly glowing coals, the rotation of the arms became so rapid as to render them almost invisible.

Upon taking the instrument out of doors between five and six o'clock in the afternoon, the thermometer a few degrees below freezing-point, the arms revolved slowly from right to left as usual, but upon bringing it near to a mass of snow, and shading the light off by some pieces of wood, I could see that the arms revolved slowly in the opposite direction, that is, in the same direction as the hands of a watch. Later in the evening I held the instrument in the open air in bright moonlight, the thermometer being at 24° F., and the rotation was again in the same direction as the hands of a watch. The next morning, when the temperature was nearly the same, but the air foggy with only feeble light, the arms revolved at about the same rate but in the usual direction, from right to left. In the evening I again held the radiometer in the moonlight in the cold frosty air; the rotation was as before, from left to right. Carrying the instrument in my hand I approached the house, the hall door of which stood open. As I came within reach of the light and heat the rotation diminished, and at length ceased, but upon entering the hall it commenced again, only in the opposite direction. In fact, I could stand in such a position that upon moving a few feet either way, I reversed the direction of rotation, while between the two there was no motion at all.

I afterwards repeated the experiment in a different form. I placed the instrument in a cupboard in a very cold room, with a considerable quantity of ice. Upon just opening the cupboard door and peeping in, I could see that the arms were revolving very slowly, but distinctly, from left to right. Upon opening the door a little wider the motion ceased, and when still more light was admitted the motion was reversed. I then removed the ice and nearly closed the door—the rotation ceased entirely; but upon introducing a piece of heated iron the arms spun round as fast as they usually do in full sunlight, and this, be it remembered, when the cupboard was almost dark, the door being only just sufficiently open for me to see the instrument, certainly not more than a quarter of an inch. T. N. HUTCHINSON

Rugby, Feb. 12

Since writing the above, I have been favoured with a note from Mr. Crookes, in which he points out to me that his results have been obtained by means of radiometers constructed with *pith* discs, and having no metal at all in the moving parts. In the little instrument that I have used the discs are of mica, blackened, of course, on the alternate faces, but mounted upon four metallic arms, apparently aluminium foil. Mr. Crookes observes: "I long ago gave up metallic instruments owing to their erratic movements while radiating or absorbing heat. I have mentioned this peculiarity of metallic radiometers in my papers for the Royal Society."

As this difference between the instruments used accounted, to some extent, for my obtaining results so different from those described by Mr. Crookes in the paper referred to, I felt at first that there was no further need to trouble you with these remarks, and that they had better be consigned to the waste paper basket. Upon second thoughts, however, it seems to me that there is still something that requires explanation, or, at all events, that I do not understand, in the different action of dark heat on pith only, and on mica mounted on thin metallic arms. The four arms are very fine, equally bright, and similar in all respects, hence it is difficult to see how rotation should be produced by the action of heat on the metallic parts of the apparatus. The vacuum, no doubt, is not so perfect as that obtained by Mr. Crookes with his exquisite Sprengel pump, but even this would hardly account for the "erratic movements" that I have observed.

I may add that since performing my experiments I have learnt that one of my pupils in Rugby School, Mr. H. F. Newall, has observed very similar results with a radiometer in his possession. T. N. H.

The Sailing Flight of Birds

HAVING had during several long voyages in the Pacific considerable opportunities for observing closely the flight of sailing birds, and especially of *Diomedea Melanophrys*, or "Mutton Bird," as I believe it is called by the Australians, a few suggestions on the subject may perhaps not be uninteresting to your readers.

This bird differs considerably in size from the albatross of the Cape, but as the principles of its flight are the same, the few suggestions I wish to make will apply with equal force to both species, and indeed to all sailing birds.

The *Diomedea* of the Cape it is well known can support itself in the air for a very long time without flapping its wings, and in "The Reign of Law" it is stated that "sometimes for a whole hour together this splendid bird will sail or wheel round a ship in every possible variety of direction, without requiring a single stroke of its pinions." This *may* be accurately true, but in the case of the smaller albatross I refer to, between one and two minutes, or perhaps 1,000 or 2,000 yards in space, is more approximately the limit to which the bird's power of sailing is exercised. When the flight begins after rest the bird appears to feel very considerable difficulty in rising from the sea. It runs along the surface for some distance, flaps its wings very vigorously, and continues to do this after it has left the water, until it acquires a

satisfactory velocity. Its subsequent sailing flight until it again increases its rate of speed by flapping, I would suggest to be merely a utilisation of this original *vis viva* to the utmost possible advantage, the ascending and descending movements of the bird being nothing more than a change from actual to potential energy, and *vice versâ*. Suppose, for the sake of simplicity, that the wind is dead ahead, and that the bird commences sailing horizontally with a certain *vis viva*. With this, by fixing its wings so as to present inclined planes to the direction of the wind, it is able to rise to a certain height, the velocity decreasing in some ratio to the ascent, and if the highest point capable of being reached is attained, the bird for the instant comes to rest; up to this moment the actual energy has been gradually changing into potential, and the bird gaining thereby a position of advantage. It is, however, extremely rare that this position is attained—most frequently the horizontal velocity is only partially destroyed. The planes of the wings being now changed with reference to the direction of the wind, the bird begins to descend; the potential energy is transformed into actual, and velocity is acquired, to be again changed into potential, and so on until it becomes necessary to renew it. The line of flight, therefore, of an albatross going directly against the wind consists of a series of undulations, the summits of which correspond to the instants of least relative velocities, or positions of greatest potential advantage; whilst the lowest points correspond to the instants of greatest relative velocity and least potential advantage.

During all this time *vis viva* is of course being extracted by the resistance of the wind, and the velocity after a while is so diminished that the bird loses its power of rising to a satisfactory position of advantage. It is then that flapping recommences and new power of flight is acquired. When it is remembered that the weight of a Cape albatross varies from 16 lb. to 20 lb., and the stretch of wings from 10 to 12 feet, it will be evident how great is the potential energy of such a bird at the height say of 100 feet, and also how complete is its power of utilising that energy. The question may be asked, how long will it be before 2,000 foot-pounds of work have been extracted by air moving at the rate of sixty miles an hour? for until it has been extracted, or nearly so, the sailing flight of the albatross need not cease. By means of a suitable mechanism for changing the inclination of the wing planes every few seconds, the sailing of the albatross, I believe, might be simulated without great difficulty. It is generally supposed that the stronger the wind the greater is the power of sailing-flight. In the special instance referred to, viz., that of sailing directly in the teeth of the wind, this is not the case. A good breeze is evidently better than either a very strong wind or a calm. In the one case, a too great resistance destroys the *vis viva* too rapidly; in the other, the bird suffers from a want of sufficient resistance, very much as a kite does during a calm.

In sailing in any other direction a violent wind may more or less aid the flight, and the velocity attained in some instances be enormous and very deceptive. It is this element, viz., the velocity acquired by sailing obliquely with the wind, that is so difficult for the eye to eliminate in estimating the actual power of the bird to sail against the wind. In flying with the wind, the resistance to the stroke being greater, the necessary speed may be more rapidly acquired and with fewer strokes, provided the bird has the requisite strength. But, as might naturally be supposed, sailing directly with the wind for any considerable distance is rarely or never seen, the bird not finding sufficient resistance in the air for its support.

From what has been said it will appear that the superior sailing power of the albatross, in comparison with other birds, is due—

(1) To its ability to acquire readily very great *vis viva* by means of its extremely powerful wings.

(2) To its almost perfect power of utilising this *vis viva* for the purpose of ascending or descending, *i.e.* of changing from a position of greatest actual to greatest potential energy and *vice versâ*, with least loss of power through resistance of the air.

The above implies an extraordinary rigidity as well as absence of concavity of the wings, by which the bird is enabled to hold them in their place like two rigid planes, and thereby present their surface to the wind under the most favourable circumstances possible. The tremulous movement seen at the tips appears to be nothing more than vibrations due to the want of absolute rigidity in the pinions. The above suggestions, if tenable, furnish an explanation also of the flight of the flying fish—the undulatory motion, or rising over the crest of a wave, which has puzzled so many casual observers, being merely a change of some of the *vis viva* of its flight into potential energy. This means necessarily a loss of velocity depending on the amount of rise, and implies the power of the fish to change its wing planes so as to ascend or descend. The original *vis viva* has of course been created by a preliminary rush through the water before emerging.

It will be seen from what has been said that the principle suggested, rightly or wrongly, as fully explaining the flight of the albatross, is that of a body—gifted with the most perfect power of placing itself in a position of advantage—sliding up and down inclined planes under the most perfect conditions possible.

R. A.

The Use of the Words "Weight" and "Mass"

IN the review of Dr. Guthrie's "Electricity and Magnetism" (NATURE, vol. xiii. p. 263) the following words occur in reference to Dr. Guthrie's definition of the absolute unit of electric resistance: "Here, irrespective of other considerations, there is the fundamental error of using the term *weight* instead of *mass*."

It is very unfortunate that the word "weight" is ambiguous; and that the ambiguity is actually so great as to lead to all but universal confusion of ideas. It is *not* really improper to use *weight* as synonymous with *mass*, and, had Dr. Guthrie meant to refer to *mass*, his using the term weight would not have constituted any fundamental error. He would only have been using an old ambiguous word in the more authoritatively established of its two common meanings. By an Act of Parliament (18th and 19th of Victoria, Chapter 72, July 30, 1855) for the special purpose of establishing standard weights and measures, it is enacted that a certain piece of platinum referred to as a "weight of platinum" shall be denominated the Imperial Standard Pound Avoirdupois, and shall be deemed to be the only standard of weight from which all other weights and other measures having reference to weight shall be derived, computed, and ascertained. The gravity of a mass, or of a piece of matter, is not once named, or in any way referred to, in the Act as a thing for which a standard is meant to be established by that Act, nor is the word *force* or the notion of force put forward in any way in the Act. Thus the meaning attributed in the Act to the word *weight* is the same as is distinctly expressed in scientific language by *mass*.

However, on turning to Dr. Guthrie's book itself, I found a striking example of the troublesome perplexity which is involved in the ambiguity of the language in common use. A few lines below the passage touched on by the reviewer, the following sentence and appended note occur. (Text), "From the work done by the current in the experimental wire, the resistance in that wire is found, and this resistance is considered unity when the above measures are units, namely, 1 second time, 1 meter space, and 1 gram weight or force." (Note appended), "The force actually taken as unity is $\frac{1}{9{\cdot}81}$ gram, for this force acting on 1 gram for 1 second will give it a velocity of 1 meter a second." The text and the note are utterly irreconcilable. The confusion is complete.

I do not say that no one can possibly understand the subject with the common nomenclature; but I do say, from considerable experience in Glasgow University, where we are in the habit of using the absolute or kinetic system of force-measurement in *all* our calculations with the students of the Natural Philosophy Class, that it is extremely difficult to explain, with the old nomenclature, the beautiful, and in itself simple, kinetic system of Gauss, together with its connection with the gravitation system of force-measurement.

This session, however, I have found a very great simplification in adopting a suggestion of Prof. James Thomson to do away with the word *weight* altogether in cases in which its employment would involve ambiguity. He would still readily use the name, a pound weight, for the standard piece of iron or brass used in weighing; and would continue, so long as our present non-decimal system is maintained, to use the commercial term, a hundredweight of iron, meaning a certain quantity or mass of iron. But he has proposed that when we mean *mass* we should avoid the word weight as far as possible and use the word mass, and that where we mean downward force due to gravitation, called by Dr. Guthrie and his reviewer, *weight*, we should use the word *gravity*. Thus we may speak of a one pound force, or we may say "the gravity of a pound," but never "the weight of a pound." We can scarcely get rid altogether of connecting the idea of heaviness with the word weight, nor would our dictionaries at present allow us to do so; but it is quite proper to feel that, in speaking of a certain weight as being too great to